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MEDICAL AND PHYSIOLOGICAL COMMENTARIES.*

To the Editor of the Boston Medical and Surgical Journal.

SIR,—Let no one suppose that I mean to make a critical analysis of the whole of Dr. Paine's work. I wish to make some remarks upon his assertions in regard to M. Louis—and if you will allow them a place in your Journal I shall be much obliged to you. Hoping that Dr. Paine will receive these remarks with "good humor," and believe that I have "felt no cold-blooded envy of his honors,"

Boston, Sept. 1, 1840.

I remain yours very truly, H. I. B.

Dr. Paine's volumes are heavy tomes indeed, so far as size is concerned—but in examining them with reference to the special object we have in view, we have been forcibly reminded of the mushroom. We have seen other volumes equally large and containing as much dogmatism, which were intended for the destruction of heresies—but notwithstanding the zeal and learning of the opponent, the damnable heresy has risen triumphant. We have no fears that Dr. Paine's volumes will have much effect; but though we have no fears for the foundation of Louis's main ideas, viz.: 1st, the accurate observation; 2d, recording of cases; 3d, the analysis of them by tables—still we feel unwilling to allow any writer, under the pretence of examining the philosophical views of M. Louis, to traduce his character. If all that is said by Dr. Paine of the former physician of La Pitié be true, M. Louis deserves to be treated not merely as one wholly unworthy of confidence in medicine, but as an individual of a base private character. We use harsh terms, we allow—but what worse accusation can be brought against any person than that he is untrue, that he is willing, for merely selfish motives, to mislead the medical world? The whole tenor of Dr. Paine's criticisms upon Louis's writings leads to such an accusation.

Dr. Paine never suffers his reader to lose sight of the main object of his two volumes, viz., a violent attack upon the numerical, or, as he chooses to call it, the anatomical school. Consequently, there is scarcely a hundred successive pages in either volume, in which this opposition does not manifest itself—and in the second volume, a whole chapter of 134 pages is dedicated to "The writings of Louis;" and after quoting from books of every nature—some of which have as much connection

* Medical and Physiological Commentaries. By Martyn Paine, M.D. A.M. 2 vols., 8vo. pp. 716—814—1830. New York: 1840.

with the subject as Mother Goose's Melodies would have with an introduction to a dissertation on the writings of Franklin—he concludes, with a self-complacent stroking of his beard, thus—"If what we have now said of the estimation in which the fathers of medicine have been held by all learned successors should, in some measure, counteract the growing prejudice against this source of much of our best experience and many of the best principles in science, we shall consider ourselves justified in having made this defence." Oh that the venerable forms of Hippocrates, Galen and Celsus could appear and duly thank their "learned successor," Dr. Paine, who in this 19th century thinks that his mission is to defend their memories from the attacks of the "bigoted numeralists"! How much ought medicine to be grateful that its fathers have been preserved from oblivion by such a cogent writer!

But let us commence with the special object of our labors—a review of Dr. Paine's ideas upon the effect of Louis's writings, and of the numerical (or anatomical, according to Dr. P.) school.

The first sign we have of the terror of our commentator in consequence of the prevalence of the writings of the "anatomical school," appears in Vol. I, when criticizing Marshall Hall's views of venesection, for the London and French pathologists are both classed under one head, although in our opinion entirely distinct characters. However, we will not quarrel with his classification of authors. Dr. Paine quotes the following from Armstrong's Lectures on Fever. The quotation will serve to show the "generalizing" powers of the Dr., as well as his dislike for Louis. "A patient, at the point of death from acute inflammation of the pleura and lungs, was bled to the extent of 50 ounces, when he obtained no relief. If we had stopped here, in two hours the patient would have died. After abstracting six ounces more blood, syncope came on, from which he recovered convalescent." We might complain of some dogmatism here, but listen to Dr. Paine: "If this patient had been bled in an erect posture, and from both arms, and had syncope followed the loss of 15 or 20 ounces of blood, it is scarcely probable that he would have been saved. Here the importance is *fully shown* [no generalizing here, we presume], not only of abstracting a certain quantity of blood, but of obtaining a full impression from the cerebral influence, in many cases of inflammatory affections," &c. Dr. P. continues, and speaks of Marshall Hall's recommendation not to bleed to perfect syncope, as being erroneous. In a note to all this, he says that he (Dr. Paine) has known many to die "from neglect or the inefficient use of" the lancet, since our author's [Dr. Hall's] and M. Louis's works have been extensively circulated amongst us. (p. 230.)

We have a few remarks to make. Considering our commentator is so very wroth, as we shall see hereafter, at Louis's love of generalizing from one or two cases, we think it very strange that Dr. P. should not merely draw an inference from one case in regard to the effect of bleeding upon pneumonia, but likewise that the same influence is exerted upon "many cases of inflammatory affections." We really cannot see any *proof* of the truth of this proposition, though Dr. P. of course expects us to take his assertions, as he kindly consents to take Dr. Hale's assertion for the

truth of the results of 197 cases. (Vol. 2, page 690.) Again, how does he know that Dr. Hall's views are wrong? Has he tried Dr. H.'s plan and found it wanting? If so, let us know the *number* of his facts *pro* and *con*—for we are unwilling to take the assertion of any man.

We may make the same remark in reference to his cases in which death occurred from want of care in bleeding. We want to see some of his cases, in order that we may compare them with some in which bleeding was omitted, or "perhaps used inefficiently," and yet the patients recovered. We would not, however, have any one suppose that we disapprove of venesection in inflammatory diseases. Far from it. Our own experience, even if Louis's cases did not prove it, would convince us that this remedy has very great influence in alleviating the general symptoms of inflammatory diseases of the chest. But we have never seen an acute inflammation "strangled" by it.

Again. The *tongue*, that favorite organ for doctors to look wise over, and therefrom divine the state of the system, becomes next the source of trouble, because, forsooth, *according to Dr. Paine*, Louis says that the indications to be drawn from the state of the tongue are "the least important" of any. (Vol. 1, Fevers, page 238.) But we beg the reader to mark well, though this is but the commencement of misstatements, *Louis never stated this*, that we can find, in his work on typhus, or in fact in any of his works, so far as we can discover. He states, and to our mind, proves, that there is no necessary connection between the condition of the tongue and that of the stomach (Vol. 2, page 55 to 90); but that certain secondary lesions are consequent upon any febrile excitement, and lesions of the tongue are among them. Louis never even said that the indications from the tongue were *unimportant*, but merely that from the tongue alone we cannot infer so much as is usually supposed, in regard to the general system, and that it does not indicate the condition of the stomach.

The next remark we have to notice, is this:—(Vol. 1, p. 282) "We shall see that Mons. Louis and his followers have little or no confidence in the curative effects of bloodletting in pneumonia, and some other equally severe inflammations." Let us see how the facts stand. Louis says that from his own investigations he is led to believe, "1st, That bloodletting has a happy effect on the progress of pneumonia; that it *shortens its duration*; that this effect, however, is much less than has been commonly believed, but that patients bled during the first four days recover, other things being equal, four or five days sooner than those bled at a later period. 2d, That pneumonitis is never arrested at once by bloodletting, at least not on the first day of the disease. If an opposite opinion is maintained, it is because this disease has been confounded with another, or because in some rare cases the general symptoms rapidly diminish after the first bloodletting. But then the local symptoms, crepitation, &c., for the most part, continue to be developed not the less for this evacuation."^{*}

We are fully convinced, from examination of the physical signs, that the local disease goes on when the general symptoms are very much improved

^{*} Bloodletting, Bowditch's Translation, p. 68.

by venesection. Now Louis would willingly allow that by subduing the fever we do much towards saving our patient, though the local disease apparently progresses—in the same way that erysipelas progresses after the first febrile action is subdued. But we do not see why Louis or his followers should be considered as “having little or no confidence” in bloodletting, because they have less faith than some others.

Moreover, let us look at the appendix to the American edition of the pamphlet on bloodletting—and which our author quotes with approbation, though it was written exactly according to the numerical method. “But this would not be representing the subject in a light sufficiently favorable for our remedy. * * * Again—so that the advantage derived from *bloodletting* in our practice is greater than that derived from the same treatment in the hands of M. Louis.”—(*Bloodletting. Appendix by Dr. Jackson.*)

Our author progresses in his zeal, and devotes three pages (Vol. 1, p. 293) to the improprieties of the numerical school. “M. Louis, in his ‘numerical’ treatise on bloodletting, endeavors to set aside the practical results of all other eminent men, whose observations probably were not less accurate, though not reduced to a tabular form. The latter observers had found themselves more usefully employed in giving their whole time to the study of nature, and in recording general facts and general results, or in presenting examples in detail, which should most clearly illustrate the ordinary conditions of disease, and thus form the luminous basis of general principles.”

Study of nature! forsooth. We presume, then, that it was from pure folly, for a mere pastime, that Mons. Louis, after having been many years in actual practice, gave up his business and entered as clinical aid to his friend Chomel—and at La Charité devoted his days. Here was no studying of nature! It is much easier to write commentaries and talk about the study of nature, than it is to observe accurately. But really, when one denies Louis the credit of having recourse to the strictest study of nature, we must smile at the critic. He knows not the man he is dealing with.

Again—“It is the complaint of the ‘numerical school,’ that general affirmations cannot be trusted without the tabular view before us.” Dr. P. is correct in the first part of this sentence. The adherents to the numerical school say we have had full enough of such words as “very often,” “frequently,” “almost always;” we want something more definite. Give us your numbers. But numeralists will be, by no means, satisfied with *all* tables or all numbers; they wish to see a book which bears internal evidence of being supported by well-investigated facts, and facts observed, as far as possible, without bias. Many times has it been repeated by the apologists of the numerical method, that tables from ill-recorded facts will lead to error. The numeralists must have an analysis of facts recorded as they actually occurred, and at the time of their occurrence, or they will not be satisfied. Dr. P. complains that thereby the honesty of all previous medical writers is called in question. Does the chemist trust to his memory when making his different analyses? Does not the astronomer record at the moment of observation, and ana-

lyze afterwards? Is this discrediting the character of previous observers? This is what Louis wishes the physician to do. No man can remember all his cases. He recollects merely those that are most prominent; hence arise errors in his writings in the closet. But Dr. P. in conclusion, says, "this method, while it offers the general results, supposes that the figures of one man are as full of accuracy as those of another." We differ from this opinion in toto (as may be inferred from our previous remarks); and we wish not to be exclusive, but we really believe that some men are from their constitution incapable of "observing nature" accurately, and the consequence is that all tables made by such persons we should be sceptical about. Already in France such persons have arisen. Our commentator quotes from them.

We might criticize more on these pages, but we will terminate with only one assertion which our author makes—"Others who have carried out M. Louis's 'numerical method,' have come to entirely different results." Every science is progressing; especially is this true in France, where there is more intellectual activity at the present time than in any other country. One law is good until a higher one is discovered; when that higher one is found out, the lesser of course is put aside. Louis, in his preface to the work on typhus, says, "The best book is good only in relation to the epoch at which it appears, and another must be anticipated that will be more exact and more complete." This we presume every reasonable person would admit. Even now Louis would not be satisfied with the want of minuteness of some of his former observations. Moreover, the attention of the scientific world is always more or less strongly bent to one object. Every tyro now has his microscope, and hopes to learn the arcana of the human frame. In a few years this will be supplanted by another method of investigating. If, in the process of time, facts may be elicited which may clash with some of Louis's results, we ought not to blame him—much less the method by which he arrived at his results.

We now approach the most important part of our author's volumes—for it is evident that Dr. Paine regards M. Louis as the most formidable opponent of medical truth of the present era, and therefore a chapter in the second volume, of more than 100 pages, is devoted to the "Writings of Louis." Four of these pages are occupied with quotations from many authors, and such a medley we have never seen or read of before, save, as Shakspeare has it:—

"Eye of newt and toe of frog,
Wool of bat and tongue of dog,
Adder's fork and blind worm's sting,
Lizard's leg and owl's wing."

Our readers may think that we give them an incorrect idea of the matter; and the quotations, it is true, are most of them extremely good—but their inapplicability to the main subject is what we object to. Plato and Lord Byron, Hippocrates and Cowper, Baglivi and Pope, are all simmering together with Louis and his contemporaries.

Our author commences thus heroically and self-complacently.—"In approaching the works which we have selected for the subjects of this

commentary, we have been actuated by various motives. These will appear, from time to time, as we advance with our undertaking. But we may say now that we have especially in view an exhibition of the ascendancy which false philosophy may obtain, in the intricate science of medicine, at the most intellectual era of man, and to exemplify the inductive and practical results which spring from morbid anatomy when assumed as a paramount guide in pathological inquiries."

Before we pass a step farther, we wish to deny entirely the truth of the assertion, that Louis or the numerical school do make pathological anatomy a *paramount* guide in pathological inquiries; and Dr. P. must know little of the matter when he accuses Louis of thus using it. We must say that we are devoted lovers of the plan originally proposed by others, but first fully developed by Louis, viz., the Numerical Method. We hereby give in our faith, and believe that as alchemy taught much of chemistry, so medicine gained much from the early fathers of medicine; but as chemistry has made rapid strides of late years from a more philosophical method of study, so medicine will gain under the numerical method. While we thus declare our entire faith, we have a right to protest against the assertion by Dr. Paine, that we depend upon pathological anatomy for our entire knowledge of diseases. We look upon pathological anatomy as only one means of deciding the question, and not more important than symptomatology. They stand upon a par; one explains and is connected with the other, and the man who neglects either is a *one-sided* philosopher and will be wholly incapable of any general views. Louis says—"I do not fear to say that pathological anatomy has been neither too much boasted of, as some declare, nor too much depreciated, as others say, but its uses have often been little understood. It is a mode of explanation which no other can supply. It is no other thing, but it is certainly of much worth, and because it is one mode of learning about diseases, a mode of verification applicable to all diseases, it seems to me we ought not to make it a science by itself, any more than diagnosis or prognosis." In other words, Louis would use it as *one* of the means of arriving at truth, but not as the sole or fundamental one.

But we go still further, and declare that there is no one from whom we could have learned more real diffidence in anatomical alterations than from M. Louis. In his lectures and at the bed-side of the patient, he is perpetually reminding us that there is something which escapes our senses, even with the most minute investigations—and for the truth of this, we appeal to any one who has followed this author in his visits, or has had personal intercourse with him. But as our readers may wish for some more tangible proof, we quote the following remark of his when speaking of pain, loss of appetite, febrile symptoms, lassitude, &c., as precursors of all *local* diseases. "We should be obliged to refer the commencement of the disease to the period at which these symptoms first made their appearance, and to draw the conclusion that an affection wholly local in its appearance may be preceded by general symp-

* Proper method of examining a patient. Bowditch's Translation from Mem. de la Soc. Med. d'Observation, Paris, 1837. Dunglison's Med. Intel., p. 160.

toms which can neither be explained nor referred entirely to the local disease, even when they arise at the same moment."—(*Ibid.* p. 154.)

It would seem, then, that our commentator raised up nothing but a spirit; and we find him fighting as a fundamental point of the numerical method, a chimera of his own brain. This two-fold error of supposing the numerical and "anatomical" schools identical, and that the former trusts to pathological anatomy as the ground work of its system, runs through the whole of the hundred pages of criticisms. Alas! that there should be so slight a foundation for the following pathetic exclamation, when speaking of the present reputation of Louis's method, &c. "When after ages shall look back upon this dark spot on the brightest escutcheon of the world, it must be regarded without sympathy and as an act of voluntary humiliation."

But let us come to details. Dr. Paine seems to think himself called upon to defend the reputation of Chomel, and assures the reader that Chomel acted a very subordinate part, and should be in no respect associated with our author's performance in his work on fever, or typhoid affection. We presume that there are scarcely two men in Paris more intimate, and more mutually respectful, than Louis and Chomel. They are very near friends. But it remains for Dr. Paine to find out that Chomel ought to be ashamed of his co-laborer; at any rate he endeavors much to persuade his readers that Louis was the acting man and Chomel had nothing to do with the matter. Now, the facts are these. Louis, disgusted with the uncertainty prevailing in all branches of medicine, returns from the foreign country where he was settled, resigns his professional duties, enters the wards of the hospital which are under the care of his friend Chomel, determined, like a sincere seeker for truth, to record the facts he sees there with perfect indifference as to the results to which a future investigation would lead him. Chomel bids him enter and pursue his studies freely; but Chomel (notwithstanding Dr. Paine takes it upon himself to declare to the contrary) is *always the chief physician*.

In order to prove how much ashamed Chomel is of his friend, and of the work which originated in his own wards, we quote the following from his *Leçons de Clinique Medicale*, Vol. 1, which treats of typhoid fever. "In speaking of the history of this disease, upon which the labors of Messrs. Prost, Petit, Serres, Bretonneau, and *especially the model work* by Mons. Louis, have thrown much light," &c. (p. 2). In quoting Louis's results, he says, "Those which Mons. Louis has given in his *learned work* upon the subject (p. 76). Again he says—"Also in the ten cases of this kind which were collected and published by M. Louis, *this able observer*," &c. (p. 128). And finally, as if it were written especially to refute Dr. Paine, we find the following—"If we judge of them from more numerous cases observed by M. Louis, in our service [or wards] at La Charité," &c.

We perhaps have spent more time than was necessary upon this, but as our author thought it necessary to devote two pages to the subject we were unwilling to pass it by unnoticed. Moreover, there is one assertion which is wholly untrue, and as it bears upon the point, we quote it. "It is

also of constant recurrence, '*I prescribed*'—meaning thereby to state definitely that Louis prescribed. Now there is no such expression in the work on typhoid fever—and we challenge Dr. P. to cite it. Dr. Paine thinking (we know not why) this point a very important one to be settled, returns to it at the latter part of his chapter, and gives another specimen of his unfairness. We quote from page 800—"Although it is everywhere apparent that he [Louis] is alone responsible, we will now state his direct affirmation to this effect. '*We abstained from blood-letting, &c.*'" Now we have always thought that common honesty of heart would tell a man that he should look at the original text, and not trust to any translation. Dr. Paine attempts to prove Louis a liar (we are aware of the meaning of the word), by quoting a translation; whereas, if he had taken the trouble to look at the original, he would have found that he by no means could prove what he wished. "*On abstint*" are the two words in the original French. Now we appeal to any one who has the merest smattering of that language, and ask whether Dr. Paine has done rightly. It is, however, quite in accordance with the greater part of the whole chapter.

But enough of this. These are mere trifles in comparison with the false statements that follow.

Having premised thus much about Chomel, our author continues, and finds an inconsistency between the motto from Emile and the advertisement (page 636 Com.). Louis says (quoting from Jean Jacques Rousseau), "I know that truth lies in facts, and not in the mind that judges of them," &c. "The reader" (thus remarks Dr. P.) "will ultimately feel the whole import and intended force of the foregoing paragraph; and whilst our author is everywhere engaged in drawing the most unqualified generalizations from these limited observations, the reader is as constantly drawn into the belief that our author is only concerned about the exhibition of *rigorous facts*. And yet be it said that our author, to carry the only purpose which could render these '138 observations' in the least instrumental to his fame beyond the day of their promulgation, announces in his '*advertisement*' what is everywhere the final object of his analytical investigations, 'the hope of arriving at *conclusive results*.'"

The above is a fair specimen of Dr. Paine's method of quoting from our author. The following is the passage to which Dr. P. refers. "Bien que mon ouvrage ne soit pas un traité de l'affection, qu'il ne doive être considéré que comme l'exposé des faits qu'j'ai recueillis sur cette maladie,* l'espoir d'arriver à des *resultats concluants* m'a conduit comme on vient de voir à l'analyse d'un grand nombre de faits relatifs à des affections d'une autre espèce." (Although my work is not a treatise on the typhoid affection, as it ought to be considered merely an account

* We must add Louis's note to this passage, and the following is a literal translation.

"This is likewise the reason why I did not think it necessary to examine the opinions of those authors who have devoted themselves most successfully of late to the study of fevers. I would add that I could not have done so without increasing the bulk of my volume; and moreover the time for this examination is perhaps not yet come, and it would be done perhaps better by another than by myself. Nothing less than all these considerations could have prevented me from using this opportunity of rendering a just tribute of respect to my brethren, and of repaying with my thanks those who have shown so much kindness to me in their publications."

of the facts which I have observed relative to this disease, the hope of arriving at conclusive results has induced me to analyze a great number of facts relative to other diseases.)—*Pref. to Typhoid Fever.*

Now we ask, has Dr. Paine acted fairly in quoting, as he has quoted, a few words from the middle of a sentence? One would suppose, from the quotation, as given by this commentator, that Louis meant to say that he considered he had settled the whole affair so far as typhous fever is concerned—that he had arrived at conclusive results, and therefore there would be no need of further investigations. Whereas he expressly states that he does not consider his work “a treatise upon the typhoid disease,” but merely an account or summary (*exposé*) of the facts which he had observed. But let us search for Louis’s opinions upon this point elsewhere. In his preface to his volume of *Memoirs*, he says, “Although the number of facts we have collected is far from being sufficient to definitely fix this proportion, still they may aid in the attainment of this object—and if every one followed the same plan, we could discover the truth after a few years—and the same method, continued for a still greater lapse of time, would enable us to decide,” &c.* In these two quotations there is certainly sufficient deference paid to others, and not any extraordinary degree of arrogance on his part.

It must be always kept in mind that Louis collected his facts in Paris, and from them deduced his results. He did not, because he could not, observe in England or America. Hence all that Louis or any of his friends would contend for, is, that an analysis of his facts gives the results for the disease known under the name of typhus or typhoid fever in Paris.

Our commentator proceeds, and states his astonishment at finding Louis generalizing too quickly. “The first thing,” he says, “that excites our surprise, is the broad affirmation that a lesion of the glands of Peyer may be taken as the anatomical characteristic of typhoid fever, because, &c. * * * * * Here, in this second generalization, is one important foundation of our author’s renown. He had thus identified himself with an unexplored disease, and presented it as an isolated affection which may always be distinguished from the group with which it is allied by a comparatively unimportant lesion of structure. * * *

* * * But let us inquire how far our author has been sustained in the foregoing generalization by the observations which he has elicited from others. We allow that this may seem a work of supererogation to the most enlightened of the profession in Europe, but it is necessary to the purposes of this essay that the subject should be considered.” How truly condescending on the part of our learned commentator to be thus willing to instruct us simple Americans in what, were we all as learned as Dr. Paine, or as “the most enlightened of the profession in Europe,” we should doubtless be very well versed! But we will not quarrel with the self-complacency of the doctor, but proceed to criticize some of the quotations from the authors whom he cites to prove his position.

First, we shall speak of “this unimportant lesion of structure.” Among the ablest and worthiest, and the one upon whom Dr. P. rests

* *Memoires, ou Recherches Anatomico-Pathologiques, &c. par P. Ch. A. Louis. Paris: 1826.*

his greatest hopes, is Chomel. In the first place Chomel everywhere, in the volume before us,* speaks of Louis as the "savant," and "able observer," and of his work on typhous fever as "a model." And what are his results? Let us listen to Dr. Paine's *description* of them, and afterwards learn the *truth*. He says (p. 689) that Chomel "has seen the same alteration of the glands of Peyer as attend typhus, in scarlatina and other affections (*sujets morts d'affections différentes*)."

We are sorry to see such a lamentable deficiency in the fairness which we expect in one who quotes. The reader doubtless will suppose, from what we have extracted from Dr. P.'s remarks, that Chomel believes that the peculiar lesion of the intestinal follicles ascribed by Louis to the typhoid affection, can be found in many diseases. Now we deny that Chomel ever said so, or meant to be understood so to say; and we assert that he declares exactly the contrary, and that it is Dr. Paine's garbled quotation that has led the reader into error. That he has made exactly the same inferences that Louis has, we do not wish to state. What two men are there that will agree wholly, when there is any room for difference of opinion? But the differences in the present case do not affect the point under consideration. Chomel divides the anatomical lesions into those that are *constant* and those that are *accidental*. In the first class are reckoned, as is done by Mons. Louis, the lesion of the follicles of the small intestine (with this difference, that under this expression Chomel includes both Peyer's and Brunner's glands), and he continues thus: "We conclude from these researches, depending upon numerous observations, agreeing in the most important particulars with those made by Mons. Louis in Paris, and Dr. Bright in England, that the alteration of the intestinal follicles is a condition *wholly peculiar* (*tout à fait particulier*) to the typhoid affection, the different periods of which we can follow as we can *those of an abscess or a cutaneous exanthem*."—(*Leçons*, page 222). Truly this looks very much as if Chomel thought the follicular affection, in the typhoid disease, was "a comparatively unimportant lesion of structure."

But our author says, Chomel has seen the same alterations in other diseases. We deny even this. On the contrary, in the three diseases mentioned by him as having something similar, viz., cholera, phthisis and scarlatina, he makes two very evident distinctions—for instance in cholera: 1st, "There is less prominence of the follicles than in typhoid. 2d, The lesion is the same at every epoch of the disease—whereas there is a regular change in them in fever." In reference to scarlatina, Chomel says that the disease of the follicles resembles that in cholera, and therefore is not like that of the typhoid disease. Moreover, let any one peruse the cases collected by Jackson† while the cholera was raging in Paris, and he will find nothing to confirm him in the idea that in the two diseases is the same anatomical change. In cholera the congregated glands, or Peyer's patches, were not diseased in comparison with Brunner's or the isolated follicles. With regard to phthisis, Chomel makes similar statements. We have ourselves had some opportunities of comparing the intestinal lesions of

* *Leçons de Clinique Médicale*, par le Prof. Chomel. Paris: 1834.

† Cases of Cholera collected at Paris, &c., by James Jackson, Jr., 1832.

phthisis and typhus—and we must say that the idea of confounding the two when we examine the whole track of the canal, never could have been entertained by us. The anatomical differences are much more distinct than many cutaneous affections—for instance, measles and scarlatina.

But in order to lay the whole truth before the reader, we must inform him that Chomel announces that he does not agree with Louis in regarding this affection of the patches as absolutely necessary to the typhoid affection, because he thinks that, 1st, sometimes it is absent in cases where the symptoms are those of the typhoid disease; and 2d, because sometimes the severity of the symptoms does not accord with the slight lesion of the follicles. This opinion is drawn not from his own facts, but from those of others. But he seems in doubt about any previous step in the disease, and as he is ignorant he is willing to confess it, and waits until further facts are collected. Dr. Paine may think he has gained his end, and that Chomel and we both allow that Louis generalized too quickly—and that in stating the anatomical characteristic of the typhoid fever to be a lesion of Peyer's patches, we declare that the symptoms are dependent upon this change of these patches. Now let us examine Louis's works and see what he says upon the subject; and first, we must say that in stating the foundation of Louis's assertions, Dr. Paine, as usual, gives an unfair impression of his labors, and leads the reader to believe that all the cases of any disease that Louis examined in order to arrive at definite conclusions in reference to the characteristic lesion of the typhoid disease, were "50 cases of acute disease having certain other analogies, and 83 other cases where these analogies are said to have been more or less wanting." After speaking of the state of doubt in which physicians were in reference to fever—some calling it a gastro-enteritis, others a putrid adynamic, ataxic and typhoid fever, Louis continues thus: "In order to make up my mind upon a question which simple discussion would not tend to elucidate, I examined and recorded, between the years of 1822 and 1827, the histories of *all the patients* affected with acute disease, that were admitted to the hospital of La Charité in the apartments under the supervision of Mons. Chomel. During this period I obtained, with the exception of some imperfectly recorded facts, 138 observations of the typhoid fever, 50 of which related to individuals that died. I analyzed both, and in order to know, among the numerous lesions found in those who died, those that were peculiar to the typhoid affection, I compared them with the alterations observed in consequence of other acute diseases, in 83 subjects, whose cases I carefully recorded. I did the same when examining the symptoms in patients affected with the typhoid disease or any other acute affection terminating fatally, or by return of health. So that in fact I have analyzed the alteration in the viscera of 133 subjects who died, and the symptoms of nearly 900."

One would think that these facts were sufficient to enable one to come to some definite (we will not use "conclusive," as it offends our commentator so much) results. "In my analysis," continues Louis, "I have wholly left out any facts which were not sufficiently exact—and when I have

deduced any consequences, I have always kept before me this idea by the author of Emile, 'I know that truth resides in things, &c.'" In a note to this paragraph Louis informs us that he threw aside as incomplete all the "observations" made during his first eight months of devotion to these studies. One would think that the accurate examination of about 1000 cases, and the autopsies of 1-10 of them, would have enabled any accurate observer to decide whether a lesion was *unimportant* or not.

So much for Louis's data and accuracy of observation of nature.

[To be concluded next week.]

BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON, SEPTEMBER 9, 1840.

REVIEW OF DR. PAINE'S WORK.

THE elaborate article commenced in this day's Journal, necessarily excludes other communications and much of the local intelligence which usually accumulates in the course of a week. Although this review will occupy a considerable portion of the next number, those who have a relish for these searching literary operations, will find a great deal to interest them. In the course of a few weeks we are expecting to publish another paper upon the same subject, but from another source, which will be distinguished for its power. Dr. Paine, with all his learning, and influenced by the best intentions, has actually set the writing part of the profession by the ears.—We take the liberty to recommend *brevity* to all who may write for our pages upon this as well as other topics. Readers are not all equally interested in the discussion of any one subject, and long articles are always sure to be complained of by some.

Medical Prize Questions.—Some weeks have passed away since it was made known who took the Boylston Prizes for the present year; but hoping to know something about the dissertations, the names of the authors have not been noticed by us prominently, as they should have been, before this. Joseph Sargent, M.D., of Worcester, Mass., won the prize offered for the best dissertation on the "*pathology and treatment of medullary sarcoma*;" and W. W. Gerhard, M.D., of Philadelphia, one of the editors of the Medical Examiner, took the other—the subject being the "*pathology and treatment of typhus and typhoid fever*." It certainly does not redound to our credit to let strangers take off Boston gold medals, year after year. Dr. Parsons, of Providence, must have a small purse full.

Proceedings of the American Philosophical Society.—To Dr. Dunglison, we presume ourselves indebted for the printed transactions of this Society for May, June and July, for which he will please accept our thanks. Mention is made, very briefly, of the *filaria* in the aqueous humor in the eye of a horse, heretofore referred to in the Journal. "Most of

the observers," says Dr. D. "believe the entozoon to be a *filaria papillosa*—but some, a lesser *strongylus*." From the records, it appears that Dr. Hays, too, takes an active part in the deliberations of this learned Society.

Congenital Malformation of the Pupil.—A young man residing in Boston, now 20 years of age, has a malformation of the pupils of both eyes, which is thought to be quite rare. The pupil of the right eye is not unlike a key-hole in appearance, as the iris is completely divided quite down the line where the lower margin of the pupil should be, to the under edge of the cornea. In the left eye the circle of the pupil is well defined, although the iris is divided; but instead of a wide rent, as in the other, there is only a dark line running down the union of the sclerótica and cornea. By directing a strong light upon the organs, the clefts in both narrow up the aperture. His vision is not good—there being, as he described it, a "blur" over the eyes, so that he can read but a few minutes at a time.

Rumination in Man.—Man, certainly, is not naturally a ruminating animal; and yet I have seen three cases wherein that process was so complete after every meal, that it not only became a serious disease, but a most disgusting exhibition.

The first was an old farmer, who, to save time, had acquired a habit of "bolting" his food, as he termed it, then getting on horseback, and subjecting his dinner piecemeal to mastication at his leisure. Being a frugal character he eventually became rich, mixed in better society, found his acquired habit, although quite comfortable to himself, very unpleasant to others, and wished to shake it off through the aid of medicine. Many prescriptions were tried, some of an elaborate kind, without effect, and he finally abandoned them all as useless.

The second case was an hysterical female, of middle age and weakly digestion. Rumination, accompanied with flatus, followed every meal, and continued for some considerable time, rendering her unwilling, as well as unfit, to appear in society, and truly miserable; she found it checked in some degree by a little whisky, then brandy and other stimulants, but one after another they lost their effect; and although it was considerably abated by several medicines and modes of regimen, it was never completely cured.

The third is a school boy, of 12 years old, of a healthy constitution; the ruminating process began soon after every meal, but most pertinaciously after dinner. Although frequently checked for it, he would absent himself for the purpose of indulging in the re-mastication, finding it attended by a somewhat pleasurable sensation.

A scruple of carbonate of soda, with three grains of powdered ginger, were ordered after every meal, and in two days he found the propensity lessened; he could, by an exertion of deglutition, prevent it, and it seemed to disappear after 12 powders had been taken. The powders being now omitted, the rumination returned; the same means again succeeded, and after using this *simple remedy* for about three weeks, he has remained now several months quite free from this disagreeable affection.—W. Wilson, in *Lancet*.

Re-vaccination in the District of Villengen.—Of 123 re-vaccinations performed at Villengen in 1838, upon persons between the ages of 11 and 20 years, the results would go to prove that the operation was successful in the proportion of two-fifths of the whole. On the other hand, the epidemics of variola would show that, in general, persons who had been vaccinated and had arrived at the age of 20 years, are but slightly affected when attacked by smallpox; but in proportion as they grow older the smallpox becomes more serious. In old persons, although previously vaccinated, the disease is as violent as in those who have never been vaccinated at all. In the epidemic of Villengen in 1837–38, none who had been vaccinated took either the variola or varioloid, although several persons were vaccinated in the very houses in which the smallpox raged. This operation is very trifling, and never attended with any serious consequences.—*Gaz. Medicale.*

Temperature of Plants.—M. Van Beck, in repeating the experiments of Dutrochet, with the physiological needles of Becquerel and Breschet, and the galvanometer of Gourgon, has observed that the temperature of plants increases until the afternoon, that it then diminishes, disappears almost wholly during the night, and returns on the following day. The maximum of inherent heat on the 29th of September, at a quarter past one o'clock, P. M., in a young leaf of the sedum cotyledon, did not exceed 0 deg. 25 centigrade. In rainy and dull weather, the phenomenon was not so evident as in a calm and clear atmosphere. In these experiments, M. Van Beck's results differed from those of Dutrochet, in finding the living leaf of a lower temperature than the dead leaf of the same plant, when the observations were conducted in the air. When made in an atmosphere impregnated with watery vapor, and beneath a bell glass, the heat of the living leaf was the greatest. Dutrochet explains this want of agreement by reference to his mode of treatment of the withered leaf. After destroying the vitality of the leaf by immersing it in hot water, he dips it immediately into cold water, and keeps it well moistened during the experiment, so that an evaporation equal to that of the living leaf may be continued. If M. Van Beck allow the leaf after immersion in hot water to dry gradually, the evaporation will have ceased, and the temperature in the living leaf be consequently lower than that of the dead when the experiment is made.—*Abridged from the Edin. New Phil. Journal.*

Case of Tumor in the Pelvis, impeding Parturition. By J. C. W. LEVER, Esq., communicated by Dr. Merriman.—The patient who was the subject of the affection described, had been many hours in labor when Mr. Lever was called to see her. Finding a tumor as large as a fetal head, occupying the middle of the pelvic cavity, and obstructing the progress of labor, the author punctured it with a lancet, and gave exit to upwards of a pint of an oily fluid, when the tumor collapsed; the pains continuing, the head of the child rapidly advanced, and birth was effected in two hours from the operation.

The case here related bears a close resemblance to some of those described by Dr. Merriman in the 10th volume of the Medical and Chirurgical Society's "Transactions."—*London Lancet.*

Amaurosis—softening of the Optic Nerves through their entire substance and length—softening of the Thalami.—A man 37 years of age

who had been blind for 3 years, died of chronic dysentery in the Aberdeen Hospital. On post-mortem examination Dr. A. Kilgour found adhesion between the dura mater and arachnoid; the sub-arachnoid tissue hypertrophied, and the tela arachnoidea in consequence easily dissected from the pia mater over the whole surface of the brain. The optic nerves were of a pearly or slight yellowish appearance, flat and thin like bands of tape, much diminished in size, and their commissure soft with some liquid in the centre of it. They were traced of the same consistency and appearance into the thalami, which were also softer than usual.—*Ed. Med. Journal.*

TO CORRESPONDENTS.—The communications of Dr. Wheeler and of W. J. B. are received and will have early attention.

MARRIED.—In Poplin, N. H., Dr. Nathan French, of Malden Centre, Ms., to Miss Ruth S. Hook, of Poplin.—At Northborough, Dr. J. J. Johnson, to Miss Mary W. Allen.

Number of deaths in Boston for the week ending Sept. 5, 54.—Males, 29.—Females, 22.

Of consumption, 7.—Infantile, 7.—teething, 2.—bowel complaint, 5.—cholera infantum, 5.—lung fever, 1.—diarrhea, 1.—dropsy on the brain, 4.—disease of the spine, 1.—dysentery, 4.—spasmodic cholera, 1.—hydrocephalus, 1.—intemperance, 2.—ulcer in the head, 1.—hooping cough, 1.—debility, 1.—typhoid fever, 1.—jaundice, 1.—delirium tremens, 1.—inflammation of the bowels, 3.—cancer in the bowels, 2.—Sis, 1.—marasmus, 1.

ORDERS FOR GOODS FROM PARIS.

This subscriber intending to remain in Paris for a year or two, will be happy to attend to any orders, of large or small amount, for physicians or others, who may be in want of books, instruments, minerals, &c. &c. Reference to Dr. Martin Gay, and Nathan Hale, Esq. Directions may be left with Mr. Foster, at the Courier Office, 4 Thorndike's building, Congress square, Boston.

Sept. 5.

S 9—3

J. H. BUCKINGHAM.

REGISTER OF THE WEATHER,

Kept at the State Lunatic Hospital, Worcester, Ms. Lat. 42° 15' 49". Elevation 483 ft.

1846. August.	THERM.			BAROMETER.			Wind, 2, P.M.	Weather, 2, P.M.	Remarks.
	h a m	a m	a m	h a m	a m	a m			
1 Satur.	70	76	60	29.56	29.55	29.46	N E	Rain	
2 Sun.	64	74	72	29.52	29.53	29.46	S E	Fair	Showery.
3 Mon.	69	79	69	29.41	29.35	29.33	S E	Fair	High wind. Fine shower.
4 Tues.	66	82	74	29.34	29.29	29.33	S W	Fair	High wind. Showers in the night.
5 Wed.	70	77	76	29.23	29.21	29.30	N W	Fair	Fine shower in the night.
6 Thur.	59	83	70	29.34	29.23	29.21	S W	Fair	High wind. Showers in the night. Some (thunder and lightning.
7 Frid.	60	71	64	29.19	29.30	29.23	S W	Fair	Shower in the afternoon.
8 Satur.	59	74	60	29.23	29.25	29.29	N W	Fair	
9 Sun.	59	73	60	29.41	29.40	29.51	N W	Fair	
10 Mon.	54	73	70	29.50	29.61	29.60	N W	Fair	
11 Tues.	56	77	71	29.60	29.53	29.45	S W	Cloudy	Severe thunder storm in the evening.
12 Wed.	61	83	72	29.42	29.39	29.30	S W	Fair	
13 Thur.	64	76	72	29.30	29.23	29.30	S E	Fair	Fine showers. Rainy night.
14 Frid.	71	76	74	29.11	29.16	29.24	S W	Fair	
15 Satur.	64	76	70	29.37	29.50	29.54	N W	Fair	
16 Sun.	54	70	64	29.65	29.72	29.73	N E	Fair	
17 Mon.	55	76	72	29.70	29.61	29.50	S	Fair	Morning foggy.
18 Tues.	60	78	72	29.61	29.60	29.75	S E	Fair	
19 Wed.	63	82	75	29.60	29.61	29.56	S W	Fair	Morning foggy. Aurora borealis.
20 Thur.	70	84	70	29.51	29.50	29.40	W	Fair	
21 Frid.	68	87	82	29.40	29.49	29.40	S	Fair	do. do.
22 Satur.	67	83	79	29.49	29.46	29.43	S	Fair	Evening, thunder and lightning.
23 Sun.	71	84	70	29.30	29.33	29.27	S W	Fair	Morning foggy. Thunder storm P. M.
24 Mon.	60	78	73	29.23	29.29	29.31	W	Fair	
25 Tues.	64	76	68	29.35	29.40	29.43	N W	Fair	Aurora borealis.
26 Wed.	56	76	70	29.51	29.56	29.55	N W	Fair	do. do.
27 Thur.	50	77	70	29.55	29.56	29.55	S W	Fair	
28 Frid.	60	74	70	29.56	29.64	29.60	S	Fair	Morning foggy. Aurora borealis.
29 Satur.	63	78	70	29.61	29.64	29.63	S	Fair	
30 Sun.	62	76	71	29.60	29.50	29.50	S	Fair	Morning foggy.
31 Mon.	67	80	71	29.49	29.25	29.33	S	Fair	do. do.

The month of August has been favorable for vegetation—the weather warm, with frequent refreshing showers, and great uniformity of temperature. Range of barometer, from 29.11 to 29.81; thermometer, from 54 to 87.

TREMONT-STREET MEDICAL SCHOOL.

THE annual instructions of the Tremont-street Medical School, for private pupils, will commence on the first day of September, consisting of lectures and examinations in the different branches of professional study—as follows:

A course of Lectures and Examinations on Anatomy, in September and October, by Dr. Reynolds, preparatory to the Winter Lectures at the Medical College.

A course of Lectures on the Principles and Practice of Surgery, including diseases of the Eye and Ear, by Dr. Reynolds. This course consists of one hundred lectures, and is continued nine months of the year during the whole period of pupillage. Stated examinations are made in the above branches—and private examinations, if desired, of the graduating class.

Lectures and Examinations in Physiology and Pathology, with a distinct course upon Auscultation, by Dr. Holmes, who will also deliver, if time permits, a course of Lectures on Surgical Anatomy during the winter.

A course of Lectures on Midwifery and the Diseases of Women, and weekly examinations on the same branches and on Chemistry, by Dr. Storer. The above course is illustrated by practical manipulations with the manikin. Arrangements have been made to provide the pupils with obstetric cases as often as may be necessary to familiarize them with this branch of practice.

The departments of Theory and Practice of Medicine, and Materia Medica, are under the superintendence of Dr. Bigelow—who will visit the Hospital with the pupils, for practical observation of disease, and clinical instruction. The exploration of the chest in diseases of the thoracic organs, is made the subject of particular attention in these visits.

Practical Anatomy has always been a primary object in this school, and ample provision is made for a permanent supply of subjects from November to April. The teachers will avail themselves of occasional opportunities to show the pupils interesting cases in private practice—and operations in Surgery and Ophthalmic Disease. The pupils may attend daily on the practice of the physicians or surgeons of the Massachusetts General Hospital, and the Eye and Ear Infirmary.

Convenient rooms, light and fuel, are provided by the instructors.

JACOB BIGELOW,
EDWARD REYNOLDS,
D. HUMPHREYS STORER,
OLIVER W. HOLMES.

Boston, June 24, 1840.

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GENEVA MEDICAL COLLEGE.

THE Medical Lectures will commence on the first Tuesday of October, and continue sixteen weeks.

Institutes and Practice of Medicine, by	T. SPENCER, M.D., Geneva.
Obstetrics and Medical Jurisprudence, by	C. B. COVENTRY, M.D., Utica.
Anatomy and Physiology, by	JAMES WEBSTER, M.D., Rochester.
Chemistry and Pharmacy, by	JAMES HADLEY, M.D., Fairfield.
Materia Medica and General Pathology, by	JOHN DELAMATER, M.D., Saratoga Springs.
Principles and Practice of Surgery, by	FRANK H. HAMILTON, M.D., Rochester.
Demonstrator	BORNER ROGERS, M.D., Geneva.
	THOMAS SPENCER, Registrar.
	C. B. COVENTRY, Dean.

Geneva, July, 1840.

Jy 15-101

ALBANY MEDICAL COLLEGE.

LECTURES will commence on Tuesday, Nov. 24, 1840, and continue sixteen weeks.

Surgery, by	ALDEN MARCH, M.D.
Theory and Practice of Medicine, by	JAMES McNAUGHTON, M.D.
Materia Medica and Natural History, by	EDMUND EMMONS, M.D.
Anatomy, by	JAMES H. ARNOLD, M.D.
Chemistry and Pharmacy, by	LEWIS C. BEER, M.D.
Obstetrics, by	DAVID M. McLAUREN, M.D.
Institutes of Medicine, by	THOMAS HUN, M.D.
Medical Jurisprudence, by	AMOS DEAN, Esq.

Jy 29-4N

ALDEN MARCH, President.
J. H. ARNOLD, Registrar.

UNIVERSITY OF PENNSYLVANIA.—MEDICAL DEPARTMENT.

THE course of Lectures will commence on Monday, the 2d day of November, and be continued under the following arrangement:—

Practice and Theory of Medicine,	NATHANIEL CHAPMAN, M.D.
Chemistry,	ROBERT HARE, M.D.
Surgery,	WILLIAM GIBSON, M.D.
Anatomy,	WILLIAM E. HORNER, M.D.
Institutes of Medicine,	SAMUEL JACKSON, M.D.
Materia Medica and Pharmacy,	GEORGE B. WOOD, M.D.
Obstetrics and the Diseases of Women and Children,	HUGH L. HOBBS, M.D.

Clinical Lectures on Medicine and Surgery are delivered regularly at the Philadelphia Hospital (Blockley), and at the Pennsylvania Hospital, from the beginning to the end of the session.

263 Chestnut street, Philadelphia, July 15, 1840.

W. E. HORNER,
Dean of the Medical Faculty.

BORROWED BOOKS.—Persons having books belonging to Dr. Lewis, are requested to return them immediately.

A. 25.—2m

THE BOSTON MEDICAL AND SURGICAL JOURNAL is published every Wednesday, by D. CLAPP, JR., at 194 Washington St., corner of Franklin St., to whom all communications must be addressed, post paid. It is also published in Monthly Parts, with a printed cover. There are two volumes each year. J. V. C. SMITH, M.D., Editor. Price \$3.00 a year in advance, \$3.50 after three months, or \$4.00 if not paid within the year. Two copies to the same address, for \$5.00 a year, in advance. Orders from a distance must be accompanied by payment in advance or satisfactory reference. Postage the same as for a newspaper.

W. E. Horner